ROYAL ASTRONOMICAL SOCIETY.

VOL. X. *January* 11, 1850. No. 3.

Rev. R. SHEEPSHANKS, V. P., in the Chair.

Dr. Moffatt, Hawarden, near Chester;
Isaac Todhunter, Esq., St. John's College, Cambridge; and
R. L. Jones, Esq., Chester;
were balloted for and duly elected Fellows of the Society.

Mr. R. T. Paine, of Boston, U.S., having called attention to the omission in the *Nautical Almanac* of the occultations of *γ Virginis*, for the early part of 1850,* Lieut. Stratford has furnished the following correction:—

Occultations Visible at Greenwich.

	Star's Name.	Mag.	Disappearance. Sid. T.	M.T.	Angle from N.Pt. Vertex.	Reappearance. Sid. T.	M.T.	Angle from N.Pt. Vertex.
1850.			h m	h m	° °	h m	h m	° °
Jan. 31	γ Virginis	4	12 59	16 15	34 39	14 5	17 21	273 290
Mar. 27	—	–	12 51	12 31	46 49	14 2	13 41	261 277

Elements for Facilitating the Computation of Occultations of Stars by the Moon.

	Star's Name.	Mag.	Green. M.T. of App. Conj. in R.A. of Moon and Star.	At Greenwich Mean Time of Conjunction. App. R.A. of Moon and Star.	App. Decl. of Star.	Diff. of App. Decl. of Moon and Star.	Limiting Parallels.
1850.			h m s	h m s	° ′ ″	′ ″	° °
Jan. 4	γ Virginis	4	7 44 5	12 34 3·29	S. 0 37 31·1	N. 49 3	89 N. 13 N.
31	—	–	16 24 17	4·09	S. 36·2	N. 56 53	89 N. 21 N.
Feb. 28	—	–	2 45 33	4·70	S. 39·8	N. 56 23	89 N. 20 N
Mar. 27	—	–	12 46 54	4·99	S. 41·3	N. 53 48	89 N. 17 N.
Apr. 23	—	–	20 50 29	5·03	S. 41·1	N. 56 32	89 N. 21 N.
May 21	—	–	2 49 16	12 34 4·89	S. 0 37 39·7	N. 66 40	89 N. 35 N.

* This oversight arose from a mistake as to the *sign* of declination. It is only wonderful that the mode of reckoning by declination instead of polar distance does not lead to more frequent and more serious errors.

Ephemerides of the newly-discovered planets for the present year are given in the *Berliner Jahrbuch* for 1852, and will not, except in special cases, be published in the *Monthly Notices*. These ephemerides will also be given in the *Nautical Almanac* for 1853, which is on the eve of publication.

NEPTUNE.

Unnoticed Observations of Neptune.

Mr. Hind says,—"In looking over the zones of Dr. Lamont, observed at Munich in 1845 and 1846, I have found two observations of *Neptune* as a star, the *first* on 1845, October 25, in zone 338, when it passed the centre wire at $21^h\ 42^m\ 43^s{\cdot}1$, and was noted as a ninth magnitude; the *second* on 1846, September 7, in zone 379, at $21^h\ 54^m\ 24^s{\cdot}9$ called an eighth magnitude. Taking the positions of a number of the brighter stars in each zone, from Airy or Henderson, I find for the apparent positions of *Neptune:*

	Green. M.T.	R.A.	N.P.D.
	h m s	h m s	° ′ ″
1845, Oct. 25	6 40 15	21 42 42·48	104 14 23·0
1846, Sept. 7	10 1 57	21 54 44·51	103 16 21·8

HAMBURG. Equatoreal. (M. C. Rümker.)

1849.	Hamb. M.T.	R.A.	Decl.	
	h m s	° ′ ″	° ′ ″	
July 25	11 57 33·6	336 23 3·3	−10 38 2·0	Equat.
28	11 36 4·3	19 5·7	39 37·3	
30	11 51 51·0	16 28·6	40 40·2	
31	13 46 52·6	15 0·2	41 20·7	Mer. Circle.
Aug. 1	11 1 30·6	13 49·7	41 44·1	Equat.
8	11 46 31·6	336 3 57·4	45 42·2	
14	10 49 9·6	335 55 3·6	−10 49 14·5	
	Meridian Circle.			
Aug. 14	12 50 29·9	335 54 54·9	−10 49 22·9	
25	12 6 7·6	38 3·0	55 54·4	
30	11 45 56·9	30 13·0	10 59 0·2	
Sept. 4	25 46·9	22 35·4	11 1 59·9	
5	21 44·4	20 57·3	2 37·5	
7	13 40·7	17 58·2	3 47·8	
9	11 5 37·0	14 58·2	4 52·4	
13	10 49 29·3	8 57·6	7 10·4	
15	10 41 26·0	335 6 4·0	−11 8 19·0	

1849.	Hamb. M.T. h m s	R.A. ° ′ ″	Dec. ° ′ ″
Sept. 19	12 25 14·5	334 59 3·2	−11 10 57·4
24	5 13·2	53 37·5	13 1·6
25	10 1 13·2	52 22·3	13 28·1
26	9 57 11·0	51 1·8	13 56·6
27	53 10·0	49 43·9	14 26·8
Oct. 1	37 7·0	44 53·9	16 15·5
5	21 5·1	40 17·8	18 2·0
6	9 17 4·6	39 9·5	18 25·6
13	8 49 4·5	32 12·0	20 59·2
14	45 5·9	31 16·5	21 19·9
15	41 6·4	30 21·5	21 44·5
16	37 7·0	29 29·7	21 59·0
22	8 13 13·6	24 59·7	23 39·3
29	7 45 26·6	21 0·3	25 3·9
30	41 28·6	20 33·6	25 7·3
Nov. 2	7 29 36·2	19 22·4	25 41·2
12	6 50 9·6	17 29·7	26 9·8
14	42 17·8	17 29·6	26 9·1
18	26 35·8	17 54·6	25 58·2
20	18 45·6	18 19·1	25 42·0
21	6 14 50·5	18 31·1	25 37·4
26	5 55 18·1	20 19·9	24 56·5
28	47 29·9	21 13·1	24 28·4
29	43 36·3	21 16·6	24 16·7
30	39 42·3	22 16·3	24 4·8
Dec. 6	5 16 18·7		22 27·0
11	4 57 2·7	31 8·8	19 32·4
13	4 49 14·9	334 32 8·4	−11 19 6·9

METIS.

LIVERPOOL. Equatoreal. (Mr. Hartnup.)

1849–50.	Green. M.T. h m s	R.A. h m s	N.P.D. ° ′ ″	Comput^{d}.—Obs^{d}. R.A. s	N.P.D. ″	Stars of Comp. B.A.C.
Dec. 30	5 57 14·2	23 3 28·06	100 52 53·0	−0·67	−2·8	7974, 8109
Jan. 7	6 13 3·2	15 50·05	99 19 11·3	0·34	4·1	8102, 8214
13	5 55 59·4	25 23·21	98 6 53·9	0·41	3·1	8109, 8274
17	6 28 41·3	23 31 56·09	97 17 29·9	−0·36	−8·4	— —

"The observations are corrected for refraction and parallax. The computed places were deduced from Mr. Graham's ephemeris. The places of the stars of comparison are taken from the catalogue cited."

HAMBURG. Equatoreal. (M. Rümker.)

1849.	Hamb. M.T. h m s	R.A. ° ′ ″	Decl. ° ′ ″	
July 22	12 51 0·9	340 37 41·8	−17 18 4·7	
25	13 24 56·1	340 19 57·0	35 20·7	
28	12 18 38·0	339 58 44·9	17 52 56·8	
30	12 40 15·2	42 13·7	18 5 15·5	
31	13 59 59·8	32 18·9::	11 48·2::	
Aug. 1	11 26 24·8	339 25 4·4::	18 17 16·3::	
11	11 38 26·5	337 34 45·0	19 23 41·1	
13	12 59 15·5		37 17·9	Mer. Circle.
13	14 4 46·1	337 7 29·5	−19 37 28·7	

ASTRÆA.

HAMBURG. Equatoreal. (M. C. Rümker.)

1849.	Hamb. M.T. h m s	R.A. ° ′ ″	Decl. ° ′ ″	Obs.
Aug. 17	13 34 21·6	50 6 27·2	+12 45 41·9	
18	13 36 43·3	19 47·0	46 46·6	
19	14 17 45·5	33 25·2	47 45·1	
20	13 37 34·1	50 45 59·0	48 29·9	
25	13 21 45·6	51 46 25·9	51 11·4	
Sept. 11	15 6 4·0	54 21 33·9	40 32·9	5
12	11 54 6·0	26 49·5	39 30·6	4
14	13 26 45·2	38 33·2	36 28·1	12
15	11 36 37·8	54 43 18·7	34 33·0	1
20	11 22 38·4	55 3 11·9	24 14·4	12
21	11 22 9·0	5 59·1	21 48·7	14
22	11 1 29·5	8 35·2	19 14·4	3
23	12 37 48·4	10 36·6	16 36·3	15
24	16 5 19·9	12 17·3	13 39·0	14
25	11 17 26·0	13 27·0	11 21·5	5
26	11 29 56·0	14 10·8	8 26·3	5
27	11 22 10·3	55 14 36·8	12 5 22·4	17
Oct. 10	10 41 57·2	54 41 48·4	11 19 20·6	4
13	9 44 1·2	24 20·3	6 27·0	3
14	9 31 3·0	17 35·3	11 2 33·5	10
15	9 50 45·4	10 15·9	10 58 10·9	10
16	10 48 50·2	54 2 26·4	53 28·4	7
18	12 3 8·0	53 45 28·7	44 20·8	8
19	9 5 9·3	37 34·9	40 16·1	8
22	9 48 58·7	53 9 3·2	26 25·6	8
25	9 55 54·7	52 37 44·4	12 9·3	1
27	9 59 38·3	52 14 49·5	+10 2 44·9	19

1849.	Hamb. M.T. h m s	R.A. ° ′ ″	Dec. ° ′ ″	Obs.
Nov. 5	9 48 24·5	50 19 32·0	+9 20 24·2	7
6	9 48 28·6	50 5 55·3	15 59·0	10
	12 16 2·5	4 15·8	28·5	Mer. Circle.
7	8 34 9·3	49 52 44·7	9 11 40·4	8
12	8 12 53·6	48 41 34·6	8 50 17·0	8
	11 46 47·8	39 13·6	49 42·9	Mer. Circle.
14	7 57 41·2	48 12 35·6	42 2·6	3
18	11 17 28·8	47 13 2·7	26 57·0	Mer. Circle.
21	10 34 10·6	46 31 30·7	17 18·1	5
	11 2 52·8	30 54·7	3·6	Mer. Circle.
23	9 53 51·7	46 4 1·3	11 17·4	6
25	10 43 34·1	45 35 9·3	6 5·7	Mer. Circle.
26	7 41 11·0	45 25 28·8	8 3 38·7	11
Dec. 4	10 28 18·0	43 48 42·1	7 49 32·4	10
	10 0 58·6	38·1	33·8	Mer. Circle.
5	9 16 24·0	38 39·2	48 38·9	6
6	7 47 34·0	43 29 5·6	47 52·5	12
11	8 16 59·0	42 43 31·0	46 25·3	10
12	7 40 43·0	35 48·7	46 35·1	14
	9 24 37·9	6·2		Mer. Circle.
13	8 10 5·0	42 28 10·5	7 47 0·4	14
	9 20 12·2	27 36·4	46 56·9	Mer. Circle.
22	8 9 26·0	41 53 8·6	+8 2 48·1	6
1850.				
Jan. 2	6 52 15·5	41 31 50·7	+8 30 58·0	10
3	8 39 16·2	33 58·2	35 6·4	7
6	6 51 32·3	41 42 14·5	8 47 5·2	15
12	9 35 48·1	42 10 39·2	9 15 19·8	14
14	10 58 28·0	42 45 2·7	26 50·2	12
19	9 5 21·2	43 4 13·8	+9 54 3·8	10

The observations are *not* corrected for parallax.

Mean Places for January 1, 1849, *of the Stars compared with Astræa, deduced from Observations with the Meridian Circle near the Time of Comparison.*

R.A. h m s	Decl. ° ′ ″	Obs.	R.A. h m s	Decl. ° ′ ″	Obs.
2 47 14·99	+7 31 11·5		3 12 26·44	+ 8 29 6·5	
48 9·45	46 15·9	3	13 41·19	41 13·7	
49 52·08	33 36·5		16 41·30	8 29 35·7	
50 7·15	44 4·1		17 3·01	9 9 14·4	
53 46·39	33 9·7		18 59·46	9 12 8·0	2
2 54 44·06	7 52 31·2		20 21·30	10 51 52·1	3
3 1 21·14	8 6 2·2		21 15·06	9 25 58·1	3
6 36·31	25 51·8	3	21 50·83	10 51 51·4	
3 10 37·27:	+8 46 59·8:		3 21 58·50	+12 22 50·4	

R.A.	Decl.	Obs.	R.A.	Decl.	Obs.
h m s	° ′ ″		h m s	° ′ ″	
3 22 9·63	+10 48 56·4	2	3 35 12·88	+10 41 50·4	
22 13·85	9 56 11·1		35 42·96	11 21 8·8	2
22 32·55	12 24 55·7		35 53·73	8 34 4·2	
23 35·27	8 14 42·1		36 19·67	10 39 24·8	
24 13·05	10 13 6·6	3	36 24·70	9 42 26·4	
25 33·53	9 10 15·1		36 29·75	11 26 3·4	2
26 13·93	8 9 6·8		37 13·51	12 45 55·4	
26 48·94	10 32 12·2		37 52·08	10 4 20·4	
27 1·69	10 53 19·9		37 56·64	12 39 56·6	
28 18·58	9 40 55·6	5	38 18·67	11 3 40·7	
28 23·32	12 47 16·4	5	38 32·52	10 42 42·1	
28 34·90	9 17 48·6		38 33·07	11 11 46·0	
29 58·77	8 38 55·0	2	38 43·44	12 38 7·2	
30 55·81	10 35 36·0	2	39 4·04	12 2 11·1	
31 16·56	8 34 8·1	2	39 59·61	10 40 28·6	3
31 45·71	11 37 46·7		41 6·28	12 26 40·1	
31 47·97	12 42 14·7		41 23·57	12 8 12·8	
32 8·70	12 35 43·6		41 28·26	10 5 39·7	
32 10·83	13 3 37·3		42 53·06	12 35 8·4	
32 13·33	10 31 59·2		43 22·88	10 23 1·9	
32 58·68	10 39 37·7		44 33·61	9 31 23·6	
33 11·90	12 48 27·9		46 2·99	8 47 51·1	2
33 19·84	9 35 33·1		50 20·32	10 35 26·5	
34 29·17	8 8 57·3		52 19·15	12 3 34·0	
34 34·03	11 28 25·3		3 55 56·10	+12 5 17·5	
3 34 41·86	+12 34 6·4				

Mean Places for January 1, 1850.

R.A.	Decl.	R.A.	Decl.
h m s	° ′ ″	h m s	° ′ ″
2 44 50·07	+7 59 29·0	2 48 24·51	+9 30 56·8
45 43·22	8 43 12·2	48 56·37	9 33 27·0
2 47 19·72	+8 42 58·9	2 51 44·01	+9 54 7·8

The small figure at the end denotes the number of observations.
Where there is no number, only one observation has been made.

On the Phenomena attending the Disappearance of Saturn's Ring.

By the Rev. W. R. Dawes.

"The interesting phenomena attending the disappearance of *Saturn's* ring in 1848, can scarcely have failed to attract the attention of the possessors of powerful telescopes in this country; but none, I believe, of the observations made have been presented to the Astronomical Society. From Professor Bond, however, a paper appears in the *Monthly Notices* for November 1849, containing

observations made on *Saturn* with Merz's large refractor at Cambridge, U. S.; and also some inferences which he deduces from them.

"My own refractor (then at Cranbrook) being employed on a regular series of micrometrical observations, was only occasionally turned on the planet. What was noticed, however, was usually recorded at the time in my observatory journal, though without any intention of publication; for it was imagined that the results of observations with far more powerful telescopes, reflecting and refracting, would doubtless appear in the *Monthly Notices*, and render my own superfluous. On a perusal of Professor Bond's paper, however, I find that I have noticed several interesting particulars not alluded to by him; and also that the deductions I have drawn from some of the phenomena observed by both of us differ greatly from his. I, therefore, beg to present to the Society the following extracts from my journal, with some remarks upon them.

"'1848, June 30. *Saturn*. Power 252. The *ansæ* of the ring are *not quite* invisible. They are of a *deep coppery tinge;* and on the following arm is a faint satellite. I get an occasional glimpse of a similar appearance on the preceding arm, but cannot decidedly verify it. The planet is more than $2\frac{1}{2}$ hours east of the meridian. On looking again carefully with powers 252 and 163, I cannot be certain of either of the points *upon* the ring; for the *ring itself* seems *brighter* in those parts; and I question whether it be not a small portion of the ring which reflects more light. On estimating carefully, I find the part must be about *the extremity of the inner ring*, which is its brightest part.

"'July 15. The ring is visible, *with the two dots* [bright points] *on it as before.*

"'August 9. Occasionally an exceedingly faint *dusky red* line extends on each side of the shadow on the ball [being the arms of the ring].

"'August 20. The shadow of the ring on the ball is very narrow. *The arms of the ring not visible.*

"'Sept. 1. Power 163. Excessively narrow shadow of the ring on the ball. Belts strong, especially the two parallel ones in the northern hemisphere. *No trace of the ring.*

"'Sept. 2. 11^h G. M. T. Power 163. *The ring is visible* as an excessively narrow line *of nearly the same colour as the planet.* The *Nautical Almanac* gives *Sept.* 3 as the time of its reappearance. *No shadow* on the ball.

"'Sept. 3. Power 188 [an equiconvex single lens]. The ring is bright, though rather duller in colour than the planet. There is a dusky line about the same degree of shade as the belts (which are strong to-night), crossing the ball precisely in the line of the ring. It is very different from the *shadow* of the ring. Is it the *ring itself*, which, being less bright than the planet, is visible on the ball; as *Jupiter's* third and fourth satellites are visible on his

disk as they transit over it? Night fine. *Enceladus* very well seen, and watched nearly up to the preceding arm.

"'Sept. 4. Ring much brighter than last night. The dusky shade across the ball is visible, but not so distinct as it was last night. The following arm appears decidedly longer than the preceding one."

"A few days after the last observation, I went to Starfield on a visit to Mr. Lassell, and on every opportunity that offered observed *Saturn* with his powerful 20-foot reflector. The following are some of the memoranda entered in my journal.

"'Sept. 12. $11^h\ 20^m$ M.T. The ring is the finest line imaginable, yet of a pretty clear bright planetary colour.

"'Sept. 13. *No ring visible, nor shadow.*

"'Sept. 14. No ring visible. There is a very narrow dusky line [on the ball] just south of the bright equatoreal region.

"'Sept. 19. The ring is *just visible* when the planet is best seen.' [It was frequently noticed to-night during the series of observations which placed beyond doubt the satellite-nature of *Hyperion*.]

"The subsequent observations were made after my return to Camden Lodge, Cranbrook, with my $8\frac{1}{2}$-foot achromatic.

"'Oct. 6. No decided appearance of the ring on either side. $9^h\ 45^m$, *Tethys* has occulted *Enceladus*. The planet is exquisitely seen. The line across the ball is very sharply defined and black, though I thought not quite *uniformly* so; the northern edge appearing blacker than the southern. [The *obscure* surface is now turned towards the earth.]

"'Oct. 11. The arms of the ring are *frequently visible*, especially that on the *following* side, which is far more steadily seen than the other.

"'Oct. 26. Exquisite vision. Glimpses of a *division in the dark shade on the ball*.

"'Oct. 30. I have several times this evening received the impression of the dark line across the ball being *divided by an excessively fine line of light*, perhaps a trifle north of its middle. Most frequently suspected with powers 323 and 460.

"'$11^h\ 30^m$. *Enceladus* is at the end of the following arm very nearly. There is a pretty bright dot *on* the following arm, and a similar one on the preceding arm.'*

"'Nov. 21. Very fine night. *Enceladus* was seen the instant the object was brought into the field of the *double-image* micrometer, power 435; and when the image was divided, the two points were still distinctly visible; proving that the telescope will

* The night of Oct. 30 was very fine. *Enceladus* was bright with power 460, and was easily watched nearly up to contact with the following arm of the ring. Its distance from the planet's centre was measured with the parallel-wire micrometer under slight illumination of the field. At $7^h\ 16^m\ 30^s$ G.M.T. the distance was $34''{\cdot}40$ by four observations. With 460 and 658 the two bright satellites of *Uranus* were steadily seen.

shew a lucid point of *half the brightness of Enceladus*. The dark shade [on the ball] is occasionally seen *divided by a bright and excessively narrow line*. The planet bears 460 very well.

"'Dec. 5. The ring is curiously broken into portions, which look almost like small satellites. Occasionally, the planet is very well seen.

"'Dec. 20. The ring is obvious enough, but is broken, especially on the following side.

"'Dec. 21. The ring is reduced to a very fine line, occasionally appearing broken, but sometimes entire. The eastern arm is not quite so bright as the western.

"'1849, Jan. 6. The ring is scarcely ever visible, but occasionally in the best moments it may be traced nearly or perhaps quite to the extremity. But some portions of it are brighter than others.'

"At this time the earth was elevated about half a degree to the *north* above the plane of the ring; the sun being about 1°·8 to the *south* of that plane. On the 19th, the earth passed from the northern to the southern side of the plane of the ring; and the *bright* surface became visible. Cloudy weather precluded observations till the 22d.

"'Jan. 22, 7^h 12^m. *Titan* is in contact with the southern side of the following arm, and about two-thirds of its length from the edge of the planet. The *ring* is bright, though narrow. *It is very much brighter in two places similarly situated on each arm;* and they coincide with the extremities of the inner and outer ring.'

"*Titan* was distinguishable from the brighter parts noticed when observed at 7^h 12^m. It afterwards came into coincidence with those on the following arm as it moved along it."

Remarks.

"1. From the observations above detailed, it is obvious, that when the *obscure* side of the ring is turned towards the earth, it is not invisible with moderate optical power. It also appears that *its visibility diminished* as the earth approached its plane, and its edge, consequently, was turned directly towards us. This is proved by the increased *difficulty* of seeing it with my refractor on Aug. 9, compared with July 15; between which dates the minor axis of the ring had considerably diminished; by its *invisibility* on Aug. 20 and Sept. 1, when the earth was very nearly in the plane of the ring; and by the increasing *facility* with which it was observed between Oct. 11 and the end of November, during which interval the minor axis had increased. And it is still more conclusively shown by the *total invisibility* of the ring even with Mr. Lassell's 20-foot reflector on Sept. 13th, when both the earth and the sun were very nearly in the plane of the ring, and consequently the sun's light would be reflected from its *edge* with scarcely any obliquity.

"Hence it may be inferred that the *edge* of the ring reflects extremely little light, and that the visibility of the ring, when its

obscure side is turned towards the earth, does not arise from the sun's light reflected from the *edge*, but from a feeble illumination of the *obscure surface;* and that it is *this surface only* which is seen, and not the edge.

" This conclusion is supported by the appearance of *bright points* in some parts of the ring when obscurely visible; those points remaining *stationary*, and not partaking of the rotation of the ring. That inequalities should exist on the *edge* of the ring so large as to appear like satellites, even with a moderate telescope, when the edge itself, though directly turned towards us and fully illuminated, was quite invisible with a reflector of 24 inches aperture (whose illuminating power is about equal to that of an achromatic refractor of 17 inches aperture), seems to be quite inadmissible.

" It may be inferred, therefore, that the appearance of bright points in the unilluminated ring arises from the greater reflective power of some portions of its *surface:* the exterior portion of the inner ring being usually the largest and brightest of all, and visible on both arms at equal distances from the ball. The brightness of these points was not always precisely the same on each side: at some times the eastern, and at other times the western, appearing the brighter. This may readily be accounted for by supposing that the ring, at a given distance from its centre, may not possess the same degree of reflective power throughout its whole circuit: indeed, it is scarcely probable that it should; and the observed fact is in perfect harmony with the *rotation* of the ring, which the stationary appearance of irregularities on the *edge* can scarcely be.

" 2. The inquiry is interesting, *Whence is the light derived which renders the obscure surface of the ring visible?*

" On this subject Sir W. Herschel says (in a paper read before the Royal Society, Nov. 12th, 1789), ' I may venture to say, that the ring cannot possibly disappear on account of its thinness; since, either from its edge, or the sides, even if it were square on the corners, it must always expose to our sight some part which is illuminated by the rays of the sun: and that this is plainly the case we may conclude from its being visible in any telescopes during the time when others of less light had lost it, and when evidently we were turned towards the unenlightened side; so that we must either see the rounded part of the enlightened edge, or else the reflection of the light of *Saturn* upon the side of the darkened ring, as we see the reflected light of the earth on the dark part of the new moon. I will, however, not decide which of the two may be the case, especially as there are very strong reasons to induce us to think that *the edge of the ring is of such a nature as not to reflect much light.*'

" Relinquishing then the idea of the visibility of the mere edge of the ring (whether from its extreme thinness or its unreflective nature), it may be asked, ' Do the appearances warrant the conclusion, that the obscure surface is rendered visible by the reflexion to us of the light reflected upon it by *Saturn?*'

" If this were the case, might we not reasonably expect that its

colour would be somewhat similar to that of the obscure portion of the moon as enlightened by the earth? It is, however, of a totally different hue, and strongly resembles the *tarnished copper* colour frequently assumed by the moon under a total eclipse. May it not also be fairly questioned whether the ring would be so brilliantly illuminated by the reflected light of *Saturn* (which must fall but feebly on the half of the ring farthest from the sun), as to cause small portions of it to rival in brightness the satellites themselves illuminated by the direct rays of the sun: the brightest points of the ring (the eastern and western extremities) receiving reflected light from *one-half only* of the illuminated surface of *Saturn*, which would be seen from them as a *half-moon?*

"I would venture, therefore, to suggest, that the illumination of the obscure surface of the ring arises from the *refraction of the sun's light through an atmosphere surrounding each of the rings*, and thus throwing a pretty strong *twilight* upon them. During the whole time that the obscure surface was turned towards the earth, the sun was not more than two degrees below its plane; and during the period embraced by the observations, the depression scarcely ever amounted to one degree. A very moderate density of atmosphere, therefore, might suffice to produce considerable illumination of the obscure surface; much greater, probably, than the reflected light of *Saturn* could give, and of a far *ruddier tinge*, more nearly resembling that of our western sky shortly after sunset.

"3. It was occasionally observed, and on Oct. 6th it is recorded, that the dark shade on the ball was not uniformly black, its *northern* portion being *blacker* than the southern. On three unusually fine nights, viz. Oct. 26th, Oct. 30th, and Nov. 21st, this shade was seen to be divided through its whole length into two parts by an excessively fine bright line. After scrutinising the object for a long time with high powers, I remained perfectly convinced of the fact, which I can account for only by supposing that the northern and blacker portion was the shadow of the ring cast by the sun upon the ball; that the somewhat lighter shade was the ring itself seen projected upon *Saturn;* and that the bright line between the two was a portion of the equatoreal regions of the planet. The relative situations of the different bodies at the time renders, I think, such an explanation probable."

On the Past History of the Comet of Halley. By Mr. J. R. Hind.

It is well known that the periodicity of the remarkable comet which bears the name of Halley was inferred by that astronomer from a comparison of three sets of elements, calculated on observations of the comets of 1531, 1607, and 1682. When his table of cometary orbits was first published, Dr. Halley says he was content to hint at his conjectures respecting the identity of the comets of those years as having some degree of probability, and to advise pos-

terity carefully to watch for its return about the year 1758. At a subsequent period, an examination of the catalogues of ancient comets showed that three others had preceded those already mentioned, "manifestly in the same order and at like intervals of time, viz. in the year 1305 about Easter; in the year 1380, the month unknown; and, lastly, in the month of June 1456;" and on making this discovery Dr. Halley says he became much more confirmed in his former opinion. Want of data, however, prevented his ascertaining beyond doubt that the comet of 1682 had appeared in any of the three years, 1305, 1380, or 1456, and he had little or nothing but equality of intervals on which to found his belief.

M. Pingré, after collecting together the immense mass of records from which his great work the *Cometographie* is constructed, was enabled to convert into a certainty Halley's supposition respecting the appearance of his comet in 1456. Two definite observations of the position were found, the one at Vienna, mentioned in an Austrian chronicle by Ebendorffer, the other at Rome, preserved in a manuscript treatise on the comet of 1468, to which Pingré had access in the Bibliothèque du Roi at Paris. The comet was in perihelion on the 8th of June at 22^h, Julian style.

The preceding appearance was referred by Halley to 1380, but M. Laugier has shown that it took place in the autumn of 1378, under nearly the same circumstances as in 1835. The Chinese observations fix the time of passage through perihelion on November $8^d\ 18^h$.—(*Conn. des Temps*, 1846.)

In a paper on the history of the comet of Halley, published in the *Comptes Rendus* for 1846, July 27, M. Laugier has recognised this body in 760 and 451. The elements calculated by Burckhardt for the comet observed by the Chinese in 989 approached so close to that of Halley's Comet, that some suspicion of their identity has been entertained. But I believe, with these exceptions, the returns which I am about to mention as either certain or probable have not been previously noticed. The valuable details existing in the annals of China, and but recently known in Europe, enable us to trace this famous comet with a high degree of probability to the year 11 before the Christian era,—a most important circumstance, not only as regards the history of this particular comet, but as bearing on the constitution of these bodies in general. I shall merely state here the results to which I have been led by a close examination of the *Cometographies* and Chinese records, without extending this notice to an inconvenient length by the insertion of details. The data which I have had to work upon are found in the *Cometographies* of Pingré, Hevelius, and Lubienietski, Ma-tuoan-lin's catalogue of comets and extraordinary stars, and the other Chinese authorities with which we are acquainted through the labours of M. Edouard Biot.

The comet of Halley having returned to its perihelion in 1378, we may expect to find some mention of it about the year 1301; and notwithstanding the anomalous character of the results obtained by MM. Burckhardt and Laugier for the first comet of that year, I

am pretty well convinced it was no other than the comet of Halley. The Chinese account, which is tolerably definite, is exceedingly well represented by the elements of that body: assuming the perihelion passage to have occurred on October $22^d\ 16^h$ Greenwich time, the comet passed through *Gemini*, south of *Ursa Major*, and finally traversed *Serpens* and *Ophiuchus*, being lost in the twilight at the end of October or beginning of November. There is one European account, however, which is not so easily reconciled with this supposition. It is that of Friar Giles, whose description has led M. Laugier to an orbit differing considerably in the position of the line of nodes from that of Halley's Comet. Now, had this Friar Giles established for himself a reputation as an exact and consistent recorder of facts, we might justly entertain serious doubts as to the identity of the comet of 1301 with Halley's; but in his account of another comet (that of 1264) he contradicts himself so manifestly in the same paragraph, that we may fairly question his ability to describe accurately what he saw. Even in the case before us there is some confusion, and M. Laugier has been obliged to admit an alteration, which produces a contradiction in Giles's account, in order to reconcile the main part of his description with the Chinese observations.

The question stands thus: Halley's Comet should have appeared in all probability about 1301. We find in that year mention of a comet whose apparent path, according to the Chinese historians, is well represented by the elements of Halley's Comet: the only objection to the identity that can be advanced rests on an observation of latitude by an European astronomer, or, rather, astrologer, whose description of another comet has been shown to contain a manifest contradiction, and who, in the very case before us, has committed one pretty decided error. I am strongly inclined to recognise in the first comet of 1301 Halley's famous star.

It will readily be understood that I have not the slightest intention to undervalue M. Laugier's investigation on this comet; there can be no doubt that if we admit the whole narration of Friar Giles, his orbit must be retained, and in that case we must conclude that the comet of Halley passed its perihelion about the year 1301, unobserved, or, at least, unrecorded.

The preceding return of the comet took place, I think, in 1223, in the month of July, shortly before the death of Philip Augustus, as French historians state. It was seen for eight days at the beginning of July in the evening twilight. The Chinese have no mention of this comet, and it unfortunately happens that European chronicles give very vague accounts of it, so that we cannot come to any definite conclusion respecting its identity with Halley's. All that can be said is, that the few particulars we possess agree perfectly well with the position of Halley's Comet in the heavens when the perihelion occurs in July, and that the comets of the preceding and following year, observed in China, appear to have had very different elements. It is therefore most likely that our comet was in perihelion in July 1223.

In 1145 the return of the comet of Halley seems to me little less than a matter of certainty. On carrying back the elements to that epoch, and fixing the perihelion passage on April 19, the whole of the particulars left us by the European and Chinese authors are exactly represented. Its discovery in the morning twilight about April 15, its increasing brilliancy towards the end of the month, the disappearance about the first week in May, and rediscovery in the evening sky in the north-west on May 14th, and gradual fading away in *Hydra* on the 9th of June, are fully explained, the positions agreeing perfectly.

The periods of revolution between 1145 and 1456 average $77\frac{1}{2}$ years, and if we reckon backwards a like interval from 1145 we arrive at the year 1067, about which epoch we may expect to recognise the comet again.

There is vague mention in several European chronicles of the appearance of a comet at the time of the death of Constantine Ducas, in May 1067. We have no particulars, and even the reality of the comet is subject to some doubt. In 1066, a year memorable in English history as that of the Norman Conquest, a very grand and remarkable comet is recorded by nearly every chronicler and historian of the age. It was observed throughout Europe and China during the months of April and May. The elements of Halley's Comet, as they at present exist, will hardly represent these circumstances with a sufficient degree of accuracy; but if we assume the following numbers we shall have a very fair agreement between observation and calculation :—

Perihelion Passage 1066, April 1^{d} 0^{h}, Greenwich Time, Julian Style.

	°	′
Longitude of Perihelion	264	55
Ascending Node	25	50
Inclination	17	0
Least Distance	0	72

Motion Retrograde.

In this orbit the longitude of perihelion is further in advance than that of the present orbit of Halley's Comet by 30°, and the node by 17° : the perihelion distance is greater by 0·14. But are these differences to be considered beyond the limits of probability? in the lapse of so many centuries may not the planetary perturbations have produced alterations in the elements at least equivalent to those here exhibited, especially since there is good reason to conclude that the plane of the orbit of Halley's Comet formerly coincided much more closely with the plane of the ecliptic than at present, the comet being, therefore, subject to far larger perturbations than it has undergone in more recent times?

The elements which I have assumed for the comet of 1066 have been obtained partly from the observations in that year and partly from the Chinese description of a comet A.D. 141, which also agrees with the supposed period of Halley's. The *same orbit will represent the apparent paths in both years;* and I would particularly insist on this point as one of some importance in the present inquiry.

I may also remark, that it appears by no means improbable that an orbit more closely resembling that of Halley's Comet might be made to represent the circumstances recorded of the comet of 1066 with tolerable accuracy. The descriptions of this object are so confused that a good deal of uncertainty is necessarily attached to any conclusion we may deduce from the observations.

The preceding return of Halley's Comet took place, I think, in 989, and Burckhardt's calculations relative to the comet observed in China in that year strongly support this idea. The perihelion passage would occur about the 12th of September.

In 912 a comet was seen in China in the month of May in *Leo*, near the star marked χ on our charts. It was also perceived in Europe. The orbit of Halley's Comet, with very trifling alterations, will agree with the above position about May 13 or 14, if we fix the perihelion at the beginning of April.

The preceding appearance of the comet should fall about 837, in which year a most splendid comet was observed both in Europe and China. The elements of this body, calculated by Pingré, exhibit a general similarity to those of Halley's Comet, with the exception of the node, which is almost diametrically opposite. Now Pingré conjectured, as he himself states, that the comet passed the *ascending* and not the *descending* node on the 10th of April, or otherwise it could hardly be said to move in a north-west direction on the 12th, as we learn from the Chinese relation. It is clear, from the position of the tail on the former date, that the head of the comet could not have been far from the ecliptic; and it appears to me that Pingré's conclusion is unavoidable, if we take into consideration the whole of the Chinese description, notwithstanding its laxity as regards the distances of the comet from the equator and the singularity of the circumstance that a comet should have become visible in the very year when Halley's would probably return to the perihelion, presenting in every element except the node a striking resemblance to the orbit of that interesting body. The comet of 837 which figures in our catalogues was, therefore, in all probability, different from Halley's, though the position and distance in perihelion and direction of motion were the same, and the inclination of the orbit (a most uncertain element in the present case) not very widely different for the two bodies.

The Chinese annals mention another comet in May and June 837, which was probably that of Halley. It is not unlikely that successive copyists have altered the original description of the path amongst the stars, and that the comets observed in *Gemini* and *Virgo* in these months were the same. For the Chinese accounts of the apparent tracks of comets present frequent instances of want of chronological arrangement: thus, a date is occasionally mentioned as the epoch of discovery, and a position corresponding to a subsequent time immediately follows. If we may interpret the Chinese description, so as to place the discovery of Halley's Comet in *Gemini* on April 29, and to refer the positions in *Leo* and *Virgo* to that body at a subsequent date, it will be easy to reconcile the

apparent path with calculation, supposing the perihelion passage to have occurred early in April.

If the comet of *Gemini* and *Leo* was not that of Halley, probably this object was missed altogether at this return.

M. Laugier has shown in the most satisfactory manner that the observations of the comet of 760 in Europe and China are perfectly represented by the orbit of Halley's Comet, the perihelion falling on June 11. This year accords with my intervals. The return of the comet in 760 appears to me little short of a certainty.—(*Comptes Rendus*, 1846, July 27.)

In September and October 684 a comet was seen in China in the western heavens; but no further particulars are given. If Halley's Comet reached its point of least distance from the sun in October, it might have been observed in this position in September and the early part of October.

The previous return should have taken place about the year 607, and the Chinese annals have several comets in that year. I find, by actual computation, that none of them present any decided indications of identity with the one which forms the subject of these remarks, and I am therefore inclined to fix its reappearance in the following year, 608, when a comet is mentioned by Matouan-lin, though (most unfortunately) he has omitted to state the days to which his positions apply. The path attributed to this body, from *Auriga*, through the lower part of *Ursa Major*, into *Scorpio*, where the comet vanished, is precisely that which Halley's Comet must follow when the perihelion takes place in October or early in November. This circumstance, and the close agreement of intervals, appear to render it highly probable that the Chinese observed the comet of Halley in 608.

After a careful examination of the particulars related of the comets of 530 or 531, which Newton and Halley, owing to the want of precise data, recognised as that of 1680, I find the whole of them may be explained by the elements of Halley's Comet, supposing it to have been in perihelion early in November. Yet this inference is necessarily open to considerable doubt, and I am very far from insisting upon it. Of one point I have become pretty well convinced by my calculations, viz. that the comet of 530 or 531 (for the year of appearance is doubtful) was not identical with the celebrated one of 1680. Pingré seems to have suspected this, though he has endeavoured by alteration of dates or positions to show that such identity may have been possible. Where, however, we find the accounts of a comet as they stand in the original authorities reconcilable with a single orbit, it is surely unfair to alter them in any way, so as to produce an agreement with some preconceived notions.

In 451, or at an interval of about 79 years from 530, a comet was observed in Europe and China. It appeared about the time of the Battle of Chalons, when Attila was defeated by the Roman General Aetius. On May 17 the Chinese saw it near the *Pleiades*, and followed it till July 13, when it was situate near *β Leonis*.

Assuming Halley's Comet to have arrived in perihelion on July 3 at midnight, M. Laugier finds a remarkable agreement between the observed and calculated positions, and there can remain but little doubt that this body was seen in 451.

Seventy-eight years previous, or in October 373, 24th day, the Chinese mention a comet in *Ophiuchus* and *Serpens*. Suppose our comet to have been at its least distance from the sun early in November, we shall find it would be located in *Ophiuchus* on the 24th of October. The account is too vague, however, to allow of any definite conclusion.

Deducting another period of 78 years, we arrive at the year 295, and at this epoch I find recorded a comet in Ma-tuoan-lin's catalogue which has every appearance of identity with Halley's. The path assigned by this historian is exactly represented by the orbit of Halley's Comet, assumed to be in perihelion at the commencement of April. It passed through the lower part of *Ursa Major*, *Leo*, and *Virgo*, having been previously seen with the same right ascension as the constellation *Andromeda*. A comparison of the track in 295 with the paths followed by the comets in 451, 760, and 1456, will sufficiently justify the inference that the observations of the year 295 really belong to the comet of Halley.

Another interval of 77 years from this epoch brings us to the year 218, when this famous body should have visited us again, and it is an important fact that the Chinese annals mention a comet in 218, which there can scarcely be a doubt was the one in question. It was seen also in Europe shortly before the death of the Emperor Opilius Macrinus, who was killed on the 7th of June. Dion Cassius describes it as a very fearful star, and the Chinese tell us it was intensely brilliant. It passed through *Auriga*, *Gemini*, *Ursa Major*, into *Leo*, but was observed first in the morning in the eastern heavens. I fix the time of perihelion passage of Halley's Comet on April 6th, and find every circumstance recorded of the comet of 218 faithfully represented.

The preceding return fell, I think, in the year 141, and a fine comet is referred to that year by the Chinese historians. It was seen first in *Aquarius* and *Pegasus*, in the morning sky, about March 27, and about three weeks subsequently became visible in the evening, traversing *Taurus*, *Gemini*, *Leo*, &c. The elements of Halley's Comet, unaltered, do not quite agree with this track and the dates attributed to the various positions; but if we suppose the following orbit, depending on the observations of 1066 and 141 (as already mentioned), we shall have a very fair agreement:—

Perihelion, March 29·1.

	°	′	
Longitude of Perihelion	251	55	Equinox of 141.
Ascending Node	12	50	
Inclination.....	17	0	
Least Distance	0	72	

Motion Retrograde.

These elements have great resemblance to those of Halley's Comet, and it is very likely that an orbit differing still less might

suffice to produce a tolerable agreement. But few comets are recorded about this year, and none of them exhibit any indications of identity with Halley's, except that of 141.

In the catalogue of Ma-tuoan-lin we find a comet in the year 65, and another in 66, either of which may possibly have been Halley's, though I think the latter agrees better on the whole. It was discovered in January in the eastern heavens; on February 20 it had the same right ascension as the star β in *Capricornus*, and advanced to the south of *Scorpio*. These circumstances are in perfect accordance with the track followed by Halley's Comet when the perihelion passage takes place on January 26th.

The few particulars we have respecting the comet of A.D. 65 may be represented by the orbit of Halley's Comet, admitting it to have reached the perihelion on August 5th. It is possible, therefore, that the sword-shaped sign that was seen over Jerusalem at the commencement of the war which ended in the destruction of the Holy City by Titus, may have been the comet of Halley.

The most ancient, and, at the same time, one of the most certain apparitions of this body, took place in the year 11 B.C., reckoning according to the manner of astronomers. It was observed, according to Dion Cassius, under the consulate of M. Messala Barbatus and P. Sulpicius Quirinus, before the death of Agrippa, and seemed as though it were suspended over the city of Rome. The Chinese found it on the 26th of August in *Gemini;* it passed over this constellation, north of *Castor* and *Pollux*, towards *Leo* and *Virgo*, moving at the rate of 6° daily. Subsequently it passed near *Arcturus* and other stars in *Boötes* and arrived in *Ophiuchus* and *Serpens*. Fifty-six days after, August 26th, it set with π and σ *Scorpii*.

After the publication of M. Biot's valuable details in the appendix to the *Connaissance des Temps* for 1846, I attempted an orbit for this comet, and was immediately struck with the similarity of the elements to those of the comet of Halley. The only alteration necessary appeared to be a diminution of the orbital inclination, which, instead of 17°, would be more satisfactory at 8° or 10°. The Chinese description cannot be strictly followed, or we should have a very irregular path; but I am satisfied that the elements of Halley's Comet for perihelion, node, and least distance, and an inclination of 8°, will accord as well with the observations as any orbit can possibly do.

Previous to the year 11 B.C. the accounts of comets become so vague that it would be vain to attempt to carry the inquiry into more remote antiquity. I think it will be deemed a fact of considerable interest that the celebrated comet which bears the name of our countryman Halley may be traced, in a pretty satisfactory manner, as far back as the year 11 before the Christian era. For this extensive knowledge of its probable history we are mainly indebted to the records preserved in the annals of the various reigning dynasties in China.

Investigation of the Parallax of Groombridge 1830.
By M. Otto von Struve.

"I have just finished a series of observations and calculations on the parallax of *Groombridge* 1830, the star whose rapid proper motion was ascertained by Argelander in 1842. The method of observation followed by me in this research was that of measuring differences of declination between that star and two neighbouring stars; these I selected so that one of them (*b*) preceded *Gr.* 1830 about 3 minutes in time and 3′ to the north; the other (*a*) followed it by 2 minutes of time and about 0′·5 to the south: (*b*) is of the tenth, (*a*) of the eighth magnitude. The second star is that which was used by M. Faye in his determination of the parallax. My observations began in 1847, Nov. 4, near to one of the minima of parallax in declination, and were finished 1849, Dec. 2, including in that space two minima and two maxima.

"The common method of observing differences of declination by equatoreals provided with wire-micrometers is well known. The operation begins by placing the wires in the direction of the daily motion, and observing the coincidence of the two wires, or the determination of the zero-point of the micrometer. Then the preceding star is brought to bisection by the fixed wire, by help of the micrometer-screw applied to the periphery of the declination-circle. This being done, it is supposed that the tube will not change its direction in declination between the transits of the two stars; and when the second star comes to the middle of the field, the moveable wire is brought, by help of the micrometer-screw, to bisect it.

"After the first day's observations, I remarked sometimes, especially in winter when the friction on the pivots is very heavy, that after having bisected the preceding star exactly, the instrument was a little changed in its position (not more, however, than some few tenths of a second) by a spring-like action of the arm, on which the declination-screw is directly applied. This action was immediately destroyed when, after having moved the tube with that screw in one direction, I gave it, at the end of the motion, a little turn in the contrary sense. But in proceeding in this manner I found great difficulty in obtaining the bisection of the star by the wire to the utmost visible exactness. To prevent this inconvenience, I had recourse to the following method of observation:—By help of the declination-screw I placed the star, in eastern horary angles, some tenths of a second (sometimes a little more or less) to the north of the wire, in western horary angles as much to the south, and observed the moment of the first good bisection, produced by the change of the refraction in different altitudes. To a horary angle of 5 hours corresponds, in our latitude, a zenith-distance of *Groombridge* 1830 of 50°, with a change of 7°·5 for each hour. At that altitude the change of the refraction is about 2″·4 for each degree of the altitude; therefore, having placed the star about half a second from the wire, I had to wait 1·6 minutes of time till the best bisection was produced by

the change of refraction. This method of observation increases considerably the time employed, but at the same time it increases in a high degree the exactness of the results, and therefore I did not hesitate to follow this method for both stars in all cases where it was applicable.

" Near to the meridian this method of observation could not be used. Fortunately, in this position of the instrument, the motion of our refractor in declination is much smoother than in greater horary angles; therefore, in this case, I succeeded better in directing the wire on the star by the declination-screw; and in producing the bisection by turning the screw alternately in different senses, I was sure to avoid all constant errors. Nevertheless, I preferred the observations in greater horary angles; and in the later period of my observations I avoided, as much as I could, making them very near the meridian.

" I have mentioned already that I observed the moment of the first good bisection produced by the change of refraction. Owing to the trembling of the images, a good bisection will generally happen some seconds of time before the moment of exact bisection, that is, before the jumps of the star produced by the state of the atmosphere will be equal on both sides of the wire; but if I had waited for that so-called moment of best or exact bisection, I should have lost most probably a great many observations. For this reason I decided to observe the moment of first good bisection. The trembling of the images will be generally constant during two hours (the time employed for one evening's observation of *Groombridge* 1830), or at least it will change proportionally to the time. In both cases its influence will be eliminated in the difference of the two observed differences of declination, as soon as the observations are symmetrically arranged. On the contrary, in the sum of the two observed differences of declination, it will enter equally for both stars. As in our case, the preceding stars were both to the north of the following; in eastern horary angles the differences of declination observed by me ought to be a little smaller than those observed to the west of the meridian. This predicted phenomenon has been completely confirmed by experience. With only two or three exceptions, I have found the sum of the two observed $\Delta\delta$, or the differences of declination between the two stars of comparison, which ought to have been constant if they did not change their relative position in the heavens, constantly a little smaller at the east of the meridian than at the west, while, again, in their differences no trace of a constant difference is to be discovered. With a good harmony between the single determinations, the mean differences in the sum of the two $\Delta\delta$, measured on both sides of the meridian, is $0''{\cdot}31$; so that we have, for the mean effect of the trembling of the images on the bisection of one star, the value $= 0''{\cdot}08$.

" Another kind of error in the measured $\Delta\delta$ of two stars may arise, when, in the interval between the transits of both stars, the tube has changed its direction in declination by the effect of tem-

perature, of more or less hygrometric state of the atmosphere, or for any other unknown reason. If such a change takes place, its action can generally be assumed to be proportional to the time elapsed between the two transits; and consequently, the difference of the two $\Delta\delta$ for a star situated in right ascension exactly in the middle between the two stars of comparison, would be quite free from that influence. In our case, the principal star was situated at a distance of $3^{m}\cdot1$ from the preceding, and of $2^{m}\cdot0$ from the following star of comparison; therefore, to avoid the errors produced by such a change, in the supposition of its proportionality to the time, we had to divide its total amount, on both observed $\Delta\delta$, in the proportion of 2 : 3. In some instances, there is no doubt, such a change has really taken place in our instrument; but its mean effect on the sum of the two $\Delta\delta$, or in the space of more than 5 minutes, deduced from the comparison of the single determinations with their mean, does not exceed $0''\cdot23$ for one day's observation. This number, as I have deduced it, ought to be found rather too great, as it includes all accidental error of observation. Its smallness, I think, is the best proof of the steadiness of our refractor.

"The exactness of measured distances, and likewise of measured $\Delta\delta$, with the wire micrometer, depends principally on the quality of the micrometer-screw. With the purpose of examining if the screw used by me was regular in all its parts, I brought it under a powerful microscope, and, having placed the wires of the microscope at a distance equal to 5 and 10 revolutions of the screw, I examined all different parts of the screw, and found them all so nearly equal one with another, that no measurable difference could be discovered in any part of the screw. Besides, to avoid more completely all possible error produced by an irregularity of the screw, or by an error in the division of the head of the screw, I never retained on two following nights the same zero-point, but continually changed it, so that all the different parts of the screw were used.

"The mean value of one revolution of the screw, and the influence of temperature on it, had previously been carefully determined. The method followed in this determination is explained in the *Description de l'Observatoire Central de Poulkova*, p. 193. The final value of one revolution of the screw was fixed there to be $r = 9''\cdot7319 - 0\cdot00022\,t$, where t signifies the temperature given in degrees of Réaumur. This determination of the influence of temperature on the value of our screw was, however, of no great importance for the determination of the parallax of *Groombridge* 1830, as the maxima and minima of the parallax in declination for that star occur in the months of April and October. The mean of the temperatures during the observations in the months adjacent to each epoch was $-1^{\circ}\cdot1$ R. for the maximum, and $-4^{\circ}\cdot8$ R. for the minimum of the parallax; so that the action of temperature on the determination of the parallax, if its whole coefficient had been neglected, would scarcely have produced an error of $0''\cdot01$ on the final value; therefore, if a small error should yet subsist in the

indicated coefficient of temperature, its influence on the deduced parallax might be regarded as insensible.

"The coincidence of the wires, or the zero-point of the micrometer, was not subject to the least uncertainty. Every day it was determined 8 times, to make sure that no change had taken place in the position of the fixed wire. The method followed herein was to bring the moveable wire in contact with both sides of the fixed wire, and to take the mean of the two corresponding indications of the micrometer-screw. The single determinations got in this manner, and compared with their mean values, prove that the probable error of the mean, for one evening, is inferior to $0''{\cdot}01$.

"To the foregoing remarks on the methods of observation I might also add that every day, before I began a series of observations, the focus, as well of the object-glass as that of the eye-piece, was carefully adjusted to coincide with the plane of the wires. Perhaps in the first part of my observations I did not give the necessary attention to this point; and for this reason I have much more confidence in the later part of the observations, made since the autumn of 1848, than in the first part. Besides, it must be remembered that in this case, as well as in all particular inquiries of practical astronomy, where new methods of observation are used, for every observer some patience will be wanted before he can have acquired the necessary experience in the manipulation of his apparatus for that particular purpose, and before he will be quite satisfied with his observations.

"The power employed in this research was No. 2 of our refractor = 207 times. The difference of declination for the star preceding *Groombridge* 1830 being 3 minutes, a higher power could not be employed, if I wished to have both stars pass through the central part of the field, where, from the construction of our eye-piece, the images are the best defined; but I feel sure that if, in any other analogous inquiry, higher powers were to be used, the exactness of the observations would still be increased.

"A regular complete set of observations for one evening contained 8 comparisons of *Groombridge* 1830 with each of the two stars. If we denote the observed differences of declination between *Groombridge* 1830 and the star *a* by Δ, those between *Groombridge* 1830 and *b* by Δ', the comparisons were arranged in the following manner:—At first I observed, with the micrometer-screw to the south, four Δ; then, in the same position of the screw, four Δ'. This being done, the position-circle was turned 180°, so that the micrometer-screw was brought to a position opposite to the first, and then the observations were continued in the inverse order, namely, at first four Δ', and finally four Δ. This symmetrical arrangement of the observations appeared to be almost necessary, to avoid all influence of periodical changes in the mean of the results got by both stars. The reversion of the position circle was an object of high importance, as thereby all individual errors in the manner of producing the bisection by the micrometer-screw were destroyed. It must be mentioned here,

that in producing the bisection of the star by the wire, the last motion of the micrometer-screw was constantly given in the same positive sense.

"Before and after each set of four observations, as I have already stated, the zero-point was verified, to show that no change had happened in it. At first, a constant difference of about 0″·15 in the zero-point of the micrometer, when used in opposite positions of the screw, was ascertained. Afterwards I made the spring which regulates the motion of the micrometer-screw act a little more strongly, and from that time this little difference was quite destroyed.

"The total number of observations made for the determination of the parallax of *Groombridge* 1830 was about 60. From this number all those were excluded in which, by a change in the state of the atmosphere, the complete number of comparisons with both stars in the fixed order was not obtained. Besides, the first two days' observations were also excluded, as for these the method of observation was not yet quite fixed, though they agree perfectly well with the results of the adjacent observations. After these exclusions there remain 47 complete observations; these observations are so distributed that 14 belong to a negative, 33 to a positive sign of the coefficient of the parallax in declination. The greater number of the observations made at a positive sign of the parallax depends solely on the more favourable state of the atmosphere in the spring, compared with that of the autumn.

Before I enter upon the results of my research, I wish to state that the foregoing detailed methods of observation were all fixed before beginning, or in the first two days of the observations. Besides, I can fairly declare that the observations were made, through all the time, without the least preoccupation with regard to the parallax. Until I had finished the whole series of observations, I had calculated nothing about the results, and I avoided even to think whether the maximum or the minimum of the parallax was to happen in the spring or in the autumn.

After having cleared the observed $\Delta\delta$ of refraction, aberration, precession, and nutation, they were reduced to the mean epoch of my observations, 1848, Sept. 20. In this reduction I used the proper motion in declination $= -5''\cdot782$, deduced by M. Peters from the comparison of his own observations with those made by Lalande, Groombridge, Bessel, and Nicolai. Then the equations of condition were formed and twice resolved by the method of least squares, with regard to the following two conditions:—

1. That the observed little differences in the sum of the two $\Delta\delta$ were merely a consequence of the method of observing the first bisection on the place of the best bisection, and that the apparent irregularities in this sum could be attributed to accidental errors of observation.

"2. That these differences were produced by little periodical changes in the direction of the tube, and consequently were pro-

portional to the intervals between the transits of each compared couple of stars.

"Both conditions could not be well united together in one system of equations, as I found some difficulty in separating exactly the qualities of their influence, especially as I had not indicated in my journal on what days I had observed the bisection produced by refraction or by direct motion of the tube.

"The resolution of both systems of equations, I. and II., gave the following results :—

I. { The parallax of *Groombridge* 1830 $= +0''.005$, with a probable error $= 0''.033$
   { The proper motion in declination $= -5'.768$,, $= 0'.026$

II. { The parallax of *Groombridge* 1830 $= +0'.051$, with a probable error $= 0'.028$
    { The proper motion in declination $= -5'.757$,, $= 0'.023$

"The probable error of the result for the difference in declination between *Groombridge* 1830 and the middle of the two compared stars obtained by one evening's observations was found to be $= 0''.078$. This number, however, corresponds only to the later period of my observations. In the first period the probable error for the result of one evening is about $0''.12$. According to this difference of the probable error in the resolution of the equations, the weight attributed to the observations of the first period was 0·4 of that belonging to the later period. It may be mentioned here that the use of different weights for the observations made in the different periods was of no consequence on the result for the parallax. The resolution of the equations I. and II., with the same weight for all observations, gave the parallax (I.) $= +0''.009$, (II.) $= +0''.053$, both values according with the preceding two results within $0''.004$.

"Neither one nor the other of the preceding two results can be regarded as quite exact, but the most exact value of the parallax following from my observations must lie between them both. However, I am inclined to give the preference to the second result, as by the method followed in the formation of the equations of condition a somewhat lower weight was attributed to the comparisons with the preceding star. This appears to be quite according with the nature of the observations, as, from the faintness of the star *b*, the observations of it in the full illuminated field were very difficult when the state of the atmosphere was not quite favourable; and if I was obliged, for this reason, to illuminate the field less, the bisection of the stars by the micrometer-wires was less accurate. This difficulty was never felt in the observations of the star *a*.

"The mean of the preceding two results for the parallax is $+0''.028$, with a probable error equal to that of each of the single results, or about $0''.030$. Hence it is evident that the parallax of *Groombridge* 1830 is so small, that the determination of its real numerical value is nearly out of the reach of the most perfect appliances of modern astronomy. If we do not adopt $0''.028$ as an exact value of the parallax (the probable error being greater than the deduced

quantity), at least we can say that it is nearly certain that the real value of the parallax is considerably inferior to a tenth of a second. Therefore the parallax $= + 1''{\cdot}08$, determined by M. Faye, can only be attributed to an imperfection in the method of observation adopted by that distinguished astronomer. Also the value of the parallax $= + 0''{\cdot}226$, found by M. Peters, is certainly erroneous, but it approaches near to the reality, as he had indicated for this value a probable error of $0''{\cdot}141$.

"The proper motion in declination of *Groombridge* 1830, determined by me and compared with that deduced by M. Peters from the absolute declination observed in the space of 53 years, is, I think, also a favourable proof of the quality of my observations. Both values accord within the limits of their probable errors; and the value determined by me, though only derived from two years' observations, is even superior to that of M. Peters with regard to the probable errors."

On the Computation of Ephemerides of the Moon.

By Mr. S. M. Drach.

The object of the writer is to prepare a form of Lunar Tables, slightly different from any at present in use, of which the characteristics may be described as follows:—

In the symbolical theory of the moon, the moon's longitude for any arbitrary time is necessarily expressed by sines and cosines of simple multiples of the time; and this form is retained by Damoiseau in the construction of his Lunar Tables. But Bürg and Burckhardt conceived that the calculation of ephemerides might be facilitated by using, as arguments for some following inequalities, multiples of time corrected by some preceding inequalities. Mr. Drach proposes to extend this system by correcting the multiples of the time, not (as Bürg and Burckhardt have done) by applying to them the exact amount of the preceding inequalities, but by applying to them certain multiples of the preceding inequalities. And Mr. Drach proposes to effect the multiplication of the preceding inequalities by this artifice. Suppose the multiples of time to be expressed by one graduation of the circle (*e.g.* the centesimal), and suppose the first inequalities to be expressed by another graduation (*e.g.* the sexagesimal); then, if we add to the *centesimal* argument the *numerical* value of the sexagesimal inequalities, we are adding to the argument a quantity really smaller than the amount of those inequalities. If the multiples of time had been expressed by sexagesimal graduation, and the first inequalities by centesimal graduation, then the addition of the *numerical* value of those inequalities would increase the arguments by a quantity greater than the inequalities.

The success of such an artifice must depend entirely on the skill with which its details are worked out; and, therefore, nothing more can be done at present than to describe the general principle.

An Account of some Improvements in a Speculum Grinding and Polishing Machine. By John Hippisley, F.R.A.S.

"The machine, as far as regards the general action of two excentric pins, which transmit motion to the grinding and polishing tools, is similar in principle to that adopted by Lord Rosse, and other machines previously in use.

"The improvements consist in the arrangement of the parts so as to effect cheapness and facility in the construction, with general convenience in use, and *especially* in the manner in which the polishing tool is connected with the apparatus which gives it motion.

"This connexion is made by a ball and socket-joint of a novel construction,* which, while it transmits a perfectly equable motion without jerks or irregularities to the polisher and leaves it free also to revolve about its own centre, as the friction between it and the speculum may direct, facilitates the application of counterpoise weights, so as to counterbalance in any required degree the weight of the polisher, especially in the very last period of the polishing.

"A polisher of considerable lightness has justly been deemed indispensable, and, for this purpose, wood has been used instead of metal in its construction. This material is, however, obviously liable to unsymmetrical alteration of figure, from unequal expansion and contraction by moisture and heat.

"Nor does it, it is submitted, adequately fulfil the condition required, namely, that of sufficiently removing pressure from the speculum. It appears almost demonstrable that the last finishing action of the polisher will be exerted with most advantage, to the perfection both of figure and polish, when it moves without any vertical pressure whatever. Those who are familiar with the adjustment of reflecting telescopes are aware that the slightest confinement or pressure on the speculum, when in its place in the telescope, is sufficient to impair its power of definition, in other words, to alter temporarily, in some degree, however small, its figure. A pressure, certainly far less than that which a wooden polisher will exert, is capable of producing this injurious effect.

"It is, then, to be expected, that when a speculum has been receiving its final figure under any pressure, that there will be some, however slight, recession from that figure when the pressure is removed; and, if so, the remedy obviously is to diminish *successively*, by counterpoises, the weight of the polisher as the figure and polish are advancing, till it ultimately moves, in giving the last finishing strokes, without any more pressure than the cohesion between the polished and sliding surfaces affords. It may also be expected that the lustre and perfection of the polish itself will be enhanced, inasmuch as the size of the abraded atoms will

* A plan of the ball and socket-joint accompanied the memoir, which was further elucidated by a working model. Unfortunately Mr. Hippisley's stay in London was not long enough to allow the model to be sufficiently considered.

then be at their minimum, since they diminish in a direct ratio with the diminution of pressure by the abrading surface.

"In the machine to which these observations refer, the pulley which drives one excentric rod is made to differ slightly in diameter from its fellow, with which it is connected by the endless band, which gives motion to the other excentric rod; so that the centre of the polisher, by their joint action, is constrained to describe curves, varying from nearly a straight line to ellipsoids having a minor axis equal to twice the thrust of the shorter excentric; and the parabolic figure is attained by the maintenance of a certain proportion, found experimentally, between the throws of these two excentrics. This proportion, for a speculum having an aperture of one inch for each foot of focal length, and under the action of a polisher of the same dimensions as the speculum, is one-third of the diameter of the speculum for the longer, and one-seventh for the shorter thrust; the figure of the speculum receding towards a spherical figure if that shorter thrust be diminished, or advancing to the hyperbolic curvature if it be increased.

"The counterpoises, then, it is suggested, should be added successively, and at those periods in the action of the machine when it may be considered that its tendency to give a parabolic figure is at its maximum; that is, when, by the combined action of the excentric pins, the centre of the polisher is describing the widest ellipsoids on the speculum. A bent wire is so placed, as an index, on the excentric rod that its point, traversing a scale fixed in any convenient position, shows the exact moment when these widest ellipsoids are being described: at those intervals the counterpoise-weights are successively added, so that the polisher may be considered to pass through *complete* cycles of its action under each alteration and diminution of pressure.

"Attention to these conditions has, in specula finished by the machine, apparently been entirely successful, both in obtaining an exceeding fine polish, and a figure which does not sensibly deviate from the parabolic.

"A nice attention to the quantity and purity of the water used in the polishing is also of much importance. A very convenient method, which, being self-acting, requires no particular care during the process, is to fix a small vessel on the excentric rod, with a thread depending from it, and so adjusted that the single drop which by the capillary and syphon action passes from the thread shall, at the extreme thrust of the excentric rod, be deposited at the edge of the speculum. The quantity may be nicely regulated by the use of a smaller or larger thread; and the water is secure from containing any gritty particle, being filtered in its passage through the thread.

"The machine is of an easy and cheap construction, and intended to be worked by the foot acting on a treadle, or by any other convenient motive power: the same lever apparatus which is used for counterpoising the polisher in finishing, being connected by a vertical rigid bar with the ball and socket joint of the tool,

affords means for adding any required amount of pressure in the rough grinding process, and thus that tedious part of the operation is considerably accelerated. It is also adapted for figuring lenses of large dimensions, to which it would impart, as well as to specula, surfaces approaching nearly to those of parabolic, or other required geometric curvatures, and with the addition of another mandril, or more, receiving rapid motion from the periphery of the fly-wheel, it has been used with great convenience for grinding and polishing lenses of the smallest dimensions.

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*On the Construction of a small Observatory, a new Stand for a Transit Instrument, and an Eye-piece for facilitating the Rectification of Instrumental Errors by Reflexion.** By John Drew, F.R.A.S.

"A very excellent 5-foot telescope by Dollond, which had been tested by Captain W. H. Smyth, who mentions it with approbation in the *Celestial Cycle*, came into my possession some time since. To use this instrument with advantage I constructed a small observatory, which has answered the purpose so well that I beg to offer a plan of the building for the use of such members of the Astronomical Society as may wish to have a convenient lodgment for moderate sized instruments at a small expense. I have followed, in most respects, the description given by the Rev. W. R. Dawes of his observatory, in vol. vii. No. 10, of the *Monthly Notices*, the principal difference being in the walls, which are formed of $\frac{3}{4}$-inch weather boarding, the planks overlapping about $1\frac{1}{2}$ inch; the roof only is covered with canvass outside the boards. The experience of two winters satisfies me that the structure is weather-proof.

"Before the foundation was laid, the ground was excavated to the depth of 18 inches and filled up with concrete; on this, brickwork is raised 6 inches above the ground. An oaken curb is laid on the brickwork, into which are inserted uprights $2\frac{1}{2}$ inches by 2, and 6 feet in height. These are united at the top to a corresponding curb, and outside of these are nailed the boards forming the walls.

"The observatory consists of two compartments, the equatoreal-room and the transit-room; the former is 9 feet in diameter, in the form of a duodecagon; the roof is nearly circular, and similar to that of Capt. Smyth at Bedford, with shutters opening in the side, increasing from 9 inches to 2 feet 6 inches in width. The lower curb of the roof and the upper curb of the wall are fitted with cast-iron plates, between which are the 3-inch cannon-balls which support the roof and enable it to revolve. The telescope is mounted with a polar axis having the usual adjustments: the right ascension and declination circles are each 15 inches in diameter, and read off,

* Drawings accompanied this paper, and a model of the transit and stand was presented at the same time by Mr. Drew.

the one to 4 seconds of time, the other to 1 minute of space. The shutters of the transit-room run along the ridge of a sloping roof, which is held together by iron hoops extending across the slit; these admit of being turned round so as not to interfere with distinct vision. The transit-room, which was originally occupied by a small astronomical circle, is 7 feet long by 6 wide. After I had built it, a very excellent 4-foot transit instrument, by Mr. Jones, late of Charing Cross, came into my possession, and a difficulty arose as to the manner of mounting it in so small a space. Some hints from the Rev. R. Sheepshanks enabled me to overcome the difficulty, and to construct a mounting which, while it occupies a small space, is so stable, symmetrical, and convenient, that I beg to present a model of it to the Society for future reference.

"On the concrete I laid a solid foundation of brickwork wrought with cement; embedded in this is a slab of Portland stone, 6 inches thick, 2 feet 4 four inches from north to south, 3 feet 6 inches from east to west. Two blocks are then erected on the east and west sides of this slab, 1 foot thick, and 2 feet 6 inches high; these have an open space of 1 foot 6 inches between them for the observer's legs. Another slab, of the same size and thickness as the first, is laid across these blocks. On the north and south sides of this upper slab two semicircular spaces are hollowed out, to admit of the observer's approach to the eye-piece of the transit instrument when the telescope points near the zenith. From the east and west sides of this upper slab rise the two piers, 1 foot square at the base, each of them bevelled off from the part immediately beneath the axis of the transit to a thickness of 6 inches; on these piers, which are 3 feet high, rest the plates which support the pivots of the axis of the transit instrument. The floor of the room runs over the first slab, and is independent of it; while the second slab, under which rest the legs of the observer when in a sitting posture, serves as a table for recording the observation.

"It is necessary with this mounting to make use of a diagonal eye-piece in taking transits of stars more than 55° in altitude; most of the slow-moving circumpolar stars may, however, be observed with a direct eye-piece.

"The upper slab affords facilities for correcting the adjustments by reflexion from a surface of mercury; for this purpose I use an eye-piece of a very cheap and simple construction, which enables me to arrive at the desired result with ease and precision. The wires of the transit instrument, when it is in a vertical position, are illuminated by a small mirror attached externally to the eye-piece, and moving on a hinge; by this mirror the light is thrown obliquely through an aperture in the side, and the wires illuminated; the direct and reflected images will then appear by looking down into the mercury through the telescope.

"The Beaufoy Clock, of which the Society has obligingly allowed me the use, stands on a pier in the transit-room.

"The cost of the observatory, exclusive of the instruments and their mounting, was 50*l.*

"The latitude and longitude of my station, as determined by triangulation from the Ordnance Map Office, Southampton, are as follows:—

Latitude ..... 50° 54′ 34″ North.
Longitude ..... 1 24 25·8 West.

Mr. Edward Cowper, professor of manufacturing art and engineering in King's College, University of London, presented a model with a description of an equatoreal mounting for a telescope. The peculiarity is in the polar axis. He proposes to make this a cast-iron pillar of great length and substance, resting with its lower end, which is continued a good way below the floor, in a socket, and having its upper support a little above the floor on a stone pier, through which the pillar is carried. The adjustments of the axis are made at this upper support, and above this there is a considerable length of axis quite clear.

A thimble or cap of a conical form goes over the polar axis, and is fitted to it at the top, and again a little above the floor. On the upper end of the cap the declination axis is fixed, just as in the German mounting. This cap carries the lower circle at its base, and is so long that there is comfortable room for the telescope and the head of the observer above the pier.

Mr. Cowper remarks, "that in this arrangement the telescope can pass the meridian in any position; the telescope can be directed to the pole without any impediment; the circles may be of large dimensions, and the polar axis being long, the motion is true. By treating the whole as a piece of *engineering* work, making everything as solid and strong as an engineer would do in making a crane to carry 20 tons or a 25-foot jib, the necessary stability would be easily attained."

Besides presenting a model, Mr. Cowper has sent a drawing with his description, so that there can be no difficulty in understanding the whole of the contrivance.

Mr. Dale presented the model of an instrument intended to be a substitute for the sliding rule.

This consists of one or more logarithmic spirals carefully drawn on a smooth plane, and a jointed rule resembling in form the usual sector in the case of mathematical instruments.

The fundamental property of the logarithmic spiral is, that all sectors having the same angle between the radii vectores are, in every respect, similar.

In Mr. Dale's drawing, the radius vector is supposed to increase tenfold in one revolution. The jointed rule is graduated on the inner edges to equal parts, reckoned from the centre; and the centre, on which the two parts of the rule turn, is fitted by a pin exactly upon the centre of the spiral.

To find a fourth proportional:—1st, Turn the rule round until that division of the scale on the first leg, which expresses the first term, is cut by the spiral; hold this in its place, and turn the other leg round the joint of the rule till it is also cut at the second term by the spiral. Now, carefully keeping the rule at this opening, turn the whole round till the third term on the first leg is cut by the spiral. The fourth or required term will be that division on the round leg which is cut by the spiral.

To find the square of any number, the first leg must be set to 1, and the second to the number whose square is required. If the rule be now turned through this angle, the second leg will show the square.

If a divided circle be drawn concentric with the spiral, then the square, cube, *n*th root may be found by placing the legs at one-half, one-third, one-*n*th of the angular opening between 1 and the number whose root is required.

Also, by dividing other concentric circles so that the first leg remaining at 1 or 10, the other leg cuts the spiral at the value of various functions of the arcs, it is easy to introduce trigonometrical and other functions.

Mr. Dale shows how, by using a second spiral on a different scale, the same instrument may be made to apply to a greater extent.

If this instrument should be found useful, it would probably be necessary to adopt some contrivance for preserving the angle between the legs of the rule while the whole scale is turned about.

"The spiral may be laid off by drawing radii and marking off the lengths from a table of logarithms. It can, however, be described by continuous motion: for if a wheel have a female screw cut through its axis, and this be screwed upon a radius having on it a male screw, then, on turning round the radius, the track of the wheel is a logarithmic spiral."

Mr. Rümker has sent a copy of all the observations of the moon and culminating stars which have been observed at Hamburg in the years 1848, 1849. These will be carefully preserved for reference and consultation.

ERRATUM.

Vol. x., No. 2, p. 40, *for* Leary not mounted *read* Carey not inverted.

London: Printed by G. Barclay, Castle St. Leicester Sq.